The Injurious Political Economy of Local Road Maintenance

Margaret Bock
Goucher College
margaret.bock@goucher.edu

Benjamin Blemings
Cornell University
btb77@cornell.edu

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Abstract

Despite recognition that government officials have politically motivated incentives to pursue new infrastructure construction at the expense of infrastructure upkeep, no prior research directly addresses how political incentives affect road maintenance separate from road construction. This paper investigates how local political incentives affect local road maintenance using unique data on completed road maintenance projects and differences-in-differences which leverages exogenous timing of mayoral elections. Local election cycles shift road maintenance timing and location, leading to increases in major injuries from car crashes. Three mayoral elections in one city are calculated to cost $271,869 which translates to $421,397,740 when considering other similar cities since 1960.

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I Introduction

Road construction brings benefits such as economic and employment growth (Agrawal, Galasso, & Oettl, 2017; Chandra & Thompson, 2000); however, Glaeser and Ponzetto (2018) highlight the rise of disamenities in urban areas associated with road construction. Road maintenance ensures that individuals can continue enjoying the benefits of roads and is visible so as to potentially serve as a politician's signal of competence (Mani & Mukand, 2007; Rogoff, 1990; Veiga & Veiga, 2007). Nonetheless, road maintenance is inconvenient since it can cause delays in travel time and could upset voters if they feel the funds are being used inefficiently.

The deleterious effects of road construction and maintenance (Glaeser & Ponzetto, 2018) could be considered at odds with Rogoff (1990), in which incumbents increase spending to signal competence as elections approach. The empirical literature is also inconclusive as to how political budget cycles affect road maintenance, not including road construction. Political budget cycles matter for spending on general roads (Blais & Nadeau, 1992; Veiga & Veiga, 2007), likely due to their salience to voters (Mani & Mukand, 2007). However, previous work does not investigate whether this differs by type of spending on roads, such as maintenance compared to new construction.

Being able to distinguish between the political budget cycle's separate effects on maintenance and construction is important for several reasons. First, shifts in composition, as opposed to overall spending, have been found to occur (Drazen & Eslava, 2010). Second, there are different economic effects of maintenance compared to construction (Agénor, 2009; Kalaitzidakis & Kalyvitis, 2004; Kalyvitis & Vella, 2015; Rioja, 2003). Furthermore, politicians already have differing incentives for maintenance compared to construction (Schmitt, 2015).[1] Closely related work finds that Democrat-approved, federally funded highway maintenance increases local Democrat vote shares (Huet-Vaughn, 2019). However, most road maintenance is not federally funded or organized, with 97% of roads being under the jurisdiction of state or local governments (ARTBA, 2021).

This paper answers the question, does local road maintenance depend on local political incentives? To do so, it uses temporally and spatially disaggregated data from Pittsburgh, Pennsylvania on mayor-voter political similarity and road maintenance for a differences-in-differences like ap-

[1] There are several reasons why road construction might be favored by politicians compared to road maintenance. One reason is that building new infrastructure is less complicated to complete and easier to finance. Second, new construction projects tend to be more popular with the public.

proach. This enables the estimation of the causal effect of political incentives and subsequent calculation of the costs of local elections for road maintenance.

The main result is that local political incentives influence the timing and location of local road maintenance. Compared to non-mayoral election years, wards that are politically similar to the mayor have 565.48 more meters of road maintenance completed in October, the month before votes are cast for the mayor. However, this shift in road maintenance is more than offset by the additional maintenance, 605.76 meters, that less politically similar wards see in June-August. This means there is little impact on overall maintenance, but mostly location and timing shifts which is relevant since maintenance costs are an increasing function of time (Burningham & Stankevich, 2005).

An interpretation of this shift in timing is that mayors believe that constituents see road maintenance as both a disamenity and a signal of competence, which is consistent with both Glaeser and Ponzetto (2018) and Rogoff (1990). While the results do not provide evidence in favor of a theory, they do advance the empirical literature by documenting that there are shifts in the timing of public spending on roads, particularly maintenance, for political reasons. Previous studies focus on the effects of political cycles on total size of spending on all road-related expenses and yearly timing. These results complement prior studies by showing the timing can be further shifted around within electoral years on road maintenance.

This is important to help gain a more thorough understanding of why and how political cycles matter for road-related spending, especially in light of recent evidence that road work can also be a disamenity (Glaeser & Ponzetto, 2018). These findings are relevant in the wake of the recent passage of the Infrastructure Investment and Jobs Act which provides new funds for road maintenance. Political incentives due to local election cycles may dictate when and how these funds are actually spent, causing deviations from socially optimal policy (Winston, 2013). This is especially relevant in the wake of the recent bridge collapse in the city of Pittsburgh in early 2022.[2] In addition to politics affecting local law enforcement operations by increasing ticketing on out of state voters (Makowsky & Stratmann, 2009), this paper shows that politics also affects the operations of local public works departments. While Lehne, Shapiro, and Eynde (2018) show that politics affects rural road construction in India, this paper extends this finding to maintenance in urban areas

[2] https://www.cnn.com/2022/01/28/us/pittsburgh-bridge-collapse/index.html

in the United States, demonstrating the external validity of this phenomenon. This also builds on the work of García and Hayo (2021), which shows that fiscal spending composition matters little to voters. Our results suggest that politicians believe that actual visible projects, different than mere spending numbers, can matter.

With these political shocks to road maintenance activity, the effect of road maintenance on car crashes is explored. In the exact months where certain wards experience negative shocks to road maintenance, there are also less car crashes and heavy truck accidents. In the exact months where certain roads experience positive shocks to road maintenance, there are more major injuries. The most likely interpretation is that rushing additional maintenance in certain areas leads to additional crashes and major injuries.

These results add to the literature on how exogenous shocks, stemming from law enforcement (DeAngelo & Hansen, 2014; Kantor, Kitchens, & Pawlowski, 2021; Makowsky & Stratmann, 2011), labor strikes (Bauernschuster, Hener, & Rainer, 2017), fracking (Muehlenbachs, Staubli, & Chu, 2021), and agriculture (Blemings, Bock, & Scarcioffolo, 2021), to transportation systems affect road safety by showing that political incentives also lead to more car crashes and major injuries. While smoother roads, in general, reduce traffic crashes (Bock, Cardazzi, & Humphreys, 2021), we show that creating these smooth roads with poorly timed maintenance projects has costly unintended consequences concerning road safety.

Finally, the results are used to calculate the costs of local election cycles on road maintenance. One mayoral election in Pittsburgh costs $37,662.84 in maintenance costs and $52,960.33 from major injuries in car crashes. Over the 3 election cycles in this paper, that total equals $271,869. Extending these estimates to other US cities with populations above 100,000, local mayoral elections have cost about $421,397,740 since 1960.

II Pittsburgh Government Details

Several institutional details about Pittsburgh are relevant. Pittsburgh is governed under a mayor-city council system in which the mayor acts as the head of a nine person council. Under mayor's leadership, the council decides the location and timing of road maintenance projects on city-owned roads. Although the Department of Public Works may initiate contracts to pursue road maintenance projects, the council "...has charge of and jurisdiction over all ordinances, resolutions, bills, or papers [concerning the Department of Public Works]." This means that the timing, location and budget for maintenance projects require final approval by the council and mayor (City of Pittsburgh, 2021). In this system, mayoral political incentives could affect road maintenance.

The mayor is the only executive official elected through a *city-wide* vote, meaning the mayor's constituents are located across the whole city. The city council members are only elected by votes in their respective districts, meaning the council members' constituents are only located in the members' districts.[3] Therefore, using city council votes is not appropriate for studying city-wide maintenance decisions. This is why the paper focuses on voter political similarity to the mayor.

The city of Pittsburgh is divided into 32 wards, or electoral districts. The boundaries of the wards do not change over the length of the sample. The ward-level data are discussed further in Section III.2.

III Data

The research question is how the increase in the importance of political incentives, with variation driven by the exogenous timing of mayoral election years, affects local government's provision of road maintenance. Since political incentives become more important as the time until votes are cast in the election dwindles, it is required that temporally disaggregated road maintenance be used. Temporally disaggregated road maintenance data is not simple to obtain due to its scarce public availability.[4] One city, Pittsburgh, PA, was identified where the necessary data were available. Next, these data are described.

III.1 Maintenance Projects

Pittsburgh differs from many other cities in that it publishes a detailed road paving schedule which includes the year a project started, the exact date it finished, and the type of maintenance job it

[3] The majority of the city council elections that take place during our sample period (18 out of 20) occur every two years. Deviations from this pattern are rare and only occur when there is a vacancy. Regardless, it is highly unlikely that the timing of these elections significantly influence our analysis.

[4] Most cities, if they publicly provide data on road maintenance in the first place, only record what maintenance projects are currently in progress. Historical road maintenance data is not available.

was.[5] Table 1 details project types, across which the inconvenience caused to residents varies.[6] Figure 1 shows most maintenance occurs during the middle of the year. In these warmer months of the year, there is less ice and snow precipitation which reduces danger to construction workers. Actual completed amount of maintenance could be better than the spending data typically used in this literature, because it represents actual maintenance completed. The same spending amount, on the other hand, could mean different amounts of maintenance if per-unit prices are also different.

In Figure 1, trends in aggregate maintenance activity in mayoral election years appears different than aggregate maintenance activity in non-election years. In mayoral elections years, maintenance projects decrease after the primaries in May and begin declining in August. In non-mayoral election years, maintenance increases after the primaries. There is also another maintenance increase in October which does not occur during mayoral election years.

Mayoral elections occur every four years, and this timing is set exogenously. Therefore, there is no maintenance-related reason this difference in patterns should exist. This suggests political incentives play a role; however, it is impossible to rule out that different weather, fiscal budgets, or some other factor besides political incentives is causing this discrepancy. To rule out alternative explanations besides political incentives, additional data must be used.

III.2 Ward-Level Voter Registration By Party

The additional data should be able to rule out alternative explanations for differences in road maintenance and point towards political incentives as being the mechanism causing the discrepancy across mayoral election years shown in Figure 1. One way to achieve this is to subset Pittsburgh into areas such that the the mayor faces stronger political incentives to adjust maintenance, during mayoral election years, in some areas compared to others. One explicitly political variable that is observed, which varies by time and space, is the proportion of voters that are registered as the same party as the mayor. We call this measure mayor-voter political similarity (PS). An elected

[5]The road maintenance data for the city of Pittsburgh, Pennsylvania is publicly available via the Western Pennsylvania Regional Data Center (https://data.wprdc.org/dataset/paving-schedule). There are N= 5,978 completed maintenance projects recorded in this data set for the period 2009-2017. Start date is not observed, so we use ending date to assign the work to a time period.

[6]For instance, milling and overlay is the process of drilling down and then laying down new road, whereas AC (asphalt concrete) overlay does not involve drilling down first. The drilling and pouring is likely more disruptive than pouring alone. The main analysis focuses on the heaviest types of maintenance and the results are not sensitive to the inclusion of types of maintenance that are less disruptive.

official attempting to maximize vote share will treat similar wards and dissimilar wards differently, because individuals in dissimilar wards are less likely to support that official.

Wards The proportion of voters registered as the same party as the mayor is recorded at the year-ward level. These wards, observed monthly, will be the unit of analysis for the panel data. The ward boundaries, as well as maintained and unmaintained roads, can be seen in Figure A.1.

III.2.1 Mayor-Voter Political Similarity

The measures of voter registration are annually updated after the May primary elections. This means the most politically similar and politically dissimilar wards become evident to the mayor at this time.[7] Pittsburgh's wards lean heavily Democrat as seen in Table A.1. The average percentage of voters registered as Republican is 13.17% and the median is 13.08%. While ideally there would be more competition between the two main political parties, fierce political competition is not necessary for officials to act in their best political interest.[8]

If the mayor cares about maximizing vote share, it is unclear that the mayor would care about raw proportion of Republican voters. It is more likely that the mayor looks at the voter registration numbers and notices wards that deviate closer or farther away from being similar. To model this behavior, Republican proportion of votes is converted to a within-ward z-score. Positive z-scores indicate that the ward deviated towards having a higher proportion of Democrats that year, thus becoming more politically similar. Since the research question is not about how different levels of government align politically, but how the mayor allocates maintenance to maximize vote share, this is an appropriate measure to use in the analysis.

Figure 2 displays how wards deviate from the within-ward average proportion of registered Republicans in Pittsburgh across wards across all years in the sample. Wards in orange signify relatively dissimilar wards in a given year, while navy wards represent wards that were relatively

[7] This data is publicly available in every year of the sample from the Allegheny County Election Results website at `https://www.alleghenycounty.us/elections/election-results.aspx`.

[8] Glaeser and Shleifer (2005) show mayors act according to political incentives even in the absence of fierce political competition using several case studies. In Boston in the early 1900s, James Curley was infamous for creating a non-competitive electoral environment by providing transfers to the type of people who supported him and not helping areas of the city where his political opposition lived. Even after Boston became less competitive as Curley's opposition began to move to the suburbs because of these policies, Curley still engaged in politically motivated transfers and policies that favored his supporters.

more politically similar. Mayoral election years (2009, 2013, and 2017) are shown on the diagonal. Figure 2 demonstrates adequate within- and across-ward variation in political similarity over the sample.

To facilitate the analysis, wards are grouped into politically similar and dissimilar categories based on the within-ward deviation in political similarity that is described above. As a starting point, wards are considered to be more politically similar if their within-ward deviation z-score is below -0.15 which is the sample median. This cutoff threshold will be relaxed by treating the z-score as continuous in descriptive analyses.

III.3 Sample and Descriptive Statistics

Road maintenance projects are joined to political wards using the boundaries they are geographically within. Each project has an associated date, enabling the creation of a ward-year-month level dataset. There are 32 wards over 9 years and 8 months, resulting in a sample of 2,592 observations. January-March observations are dropped, because there are 13 out of 4,669 maintenance projects completed in these months.[9]

Summary Statistics Table A.1 shows summary statistics. One-third of observations occur during the mayoral election years of 2009, 2013, and 2017. There are an average of 2.3 maintenance projects in a ward-year-month and 1.91 of them are heavy maintenance projects. Of those heavy maintenance projects, most are milling and overlay of varying depths. The 2.3 monthly maintenance projects translate into 786.45 meters of road maintenance. Data is also gathered from the Pennsylvania Department of Transportation on car crashes, and there are an average of 7 car crashes that occur on local roads. Data from 311 calls on potholes is also reported but is unavailable for most of the sample.

[9]Furthermore, only 4 of these are heavy projects which represent only 336.8826 meters of maintenance. It is highly unlikely that this has any impact on the results. December 2017 is dropped, because the majority of these projects do not finish until after the sample period ends.

III.4 Effect of Mayoral Election Years on Maintenance in Similar and Dissimilar Wards

Next, the effect of party similarity on road maintenance by mayoral election year and months until the vote happens is examined. Table 2 uses row-wise t-tests between mayoral and non-mayoral election years of the same treatment group and month. In similar wards in June, 2.64 more projects are completed in non-election years compared to election years. However, for politically dissimilar wards 2.84 fewer projects are done in non-election years in July. There is less maintenance in similar wards in June and more maintenance projects in dissimilar wards in July which suggests there is nearly 1-1 substitution of projects out of non-similar areas into similar areas during summer of election years. In October, there are 2.55 more projects completed in dissimilar districts in non-election years than election years suggesting that most of the substituted maintenance is completed, later.

In the before election months of April and May, 2 out of 4 t-tests are statistically significant at 90% confidence. This is inconsistent with the hypothesis that the mayor shifts around maintenance due to political reasons, because the mayor has not yet observed the primary results. However, an important distinction to make with these coefficients is that, for wards of the opposite political similarity type in the same month, there is a same-signed difference. If one adjusts by the same month , like regression analyses will, then there are no statistically significant differential effects of mayoral election years on road maintenance by political similarity in the pre-election period. This shows there are no differences in average maintenance, across mayoral election years, in the pre-inter election period. This is similar to the post-election period in which all ward-politically similar pairs are positively signed, although insignificant. Unlike the before and after election periods, the inter-election period has months where the effect of the mayoral election differs by political similarity.

III.4.1 Geographic Variation in Political Similarity

While the main analysis does leverage the ward location of projects, it does not take advantage of the exact location of maintenance in relation to ward boundaries. Additional data visualization is provided which leverages the discontinuous shift in political similarity across some ward boundaries that occurs in mayoral and non-mayoral elections. If there is no effect of political incentives on road maintenance decisions, then maintenance should be continuous across this ward boundary threshold that discontinuously shifts political similarity. In order to facilitate this analysis, the data is reformatted so that the unit of analysis is the 1 meter road segment by year-month level.[10,11] A dummy variable is created for every 1 meter segment to indicate if that segment was maintained for every possible year-month combination in the sample.

Figure 3 presents these data visualization plots which show average heavy maintenance, conditional on distance to a ward boundary. At the vertical line and to the right the distance is positive which means that the road segment is in a politically dissimilar ward. Figure 3a focuses on June-August in mayoral election years. There is a visible, discontinuous downward shift in the local regression fit of maintenance projects in mayoral election years that occurs at the ward boundary. This means that more politically similar areas get less maintenance in June-August of mayoral elections years. This suggests that political incentives matter, because roads that are 20 meters apart are receiving different average amounts of maintenance. Figure 3c shows the effect of changing political similarity, in non-mayoral election years, is zero because there is no discontinuous change in maintenance at the border.

Figure 3b examines the effect of political incentives at ward boundaries in October (nearest to the election) during mayoral election years. It is cold, so few maintenance projects are completed; this is why most of the distances are 0 within 150 meters of the boundary cutoff. While there is no discontinuous change right at the cutoff, there is a point to the right, around 60 meters, in which maintenance begins to occur. A traditional RD analysis would estimate no treatment effect, but in this context there is strong reason to believe that this increase in maintenance that occurs away from the border is meaningful.[12]

[10]The population of Pittsburgh-owned roads from the Western Pennsylvania Regional Data Center is used to format data this way which results in 2.936 million unique 1 meter segments. Each 1 meter road segment is joined spatially to a city ward using GIS.

[11]As is common in discontinuity-based approaches in which units right near the cutoff may be different than the population (e.g. Dague, DeLeire, and Leininger (2017)), segments within 10 meters of the border are dropped. This is done, because often roads are used as ward boundaries as shown by Figure A.2. Also, if a 1 road meter segment has portions in both wards, dropping near segments removes this source of measurement error.

[12]If political incentives had no causal effect on maintenance, then there would be no reason distance from a boundary determining political similarity would be predictive of road maintenance. Furthermore, we don't use RD to estimate the causal effects, because there are likely to be complications from a geographic RD design such as spillovers and policy interactions (Keele & Titiunik, 2015). Regardless, it is also unlikely that there are discontinuous changes in covariates exactly 90 meters from the border.

A likely reason that the increase occurs far away from the border is because drivers from politically dissimilar wards can drive on roads in more and less similar wards. It appears the mayor has taken this into account to allow for a buffer zone at the boundary. Just as with the results for June-August, this pattern that is consistent with political incentives is not observed when there is no mayoral election. Figure 3d shows that maintenance is continuous through the cutoff and there are no outlier clusters of maintenance beyond the cutoff. This visualization, consistent with Table 2, shows that more maintenance occurs in more competitive, or dissimilar, places in June-August of mayoral election years and less maintenance occurs in less competitive, or similar, places in October of mayoral election years.

III.4.2 Binned Scatterplots

So far, the descriptive data has required assuming a sharp cutoff in the political similarity score that divides wards into categories. This could be a strong assumption, so the optimally binned (Cattaneo, Crump, Farrell, & Feng, 2019) scatterplots in Figure 4 relax this assumption by examining whether mayoral elections affect wards of different political similarity differently when political similarity is continuous.[13] In Figure 4a, an increase in political dissimilarity leads to an increase in maintenance for mayoral election years (navy line) in the months of June-August. In Figure 4b, an increase in political dissimilarity leads to a decrease in maintenance for mayoral election years (navy line) in the month of October. However, when it is not a mayoral election year (orange lines in Figure 4a and Figure 4b), there is no change in the slope from positive to negative when comparing June-August to October. The change in slope is consistent with the mayor changing road maintenance allocation based on constituent political similarity to maximize votes as the time of the election draws closer.

Figure 4 reaches the same conclusions as the t-tests (Table 2) and the geographic discontinuity plots (Figure 3) without making any assumptions about the appropriate cutoff value for being considered politically similar. Furthermore, these effects cannot be attributed to confounding from weather, the municipal budget, or input prices, since these are all constant across wards. While this approach is not vulnerable to these possible confounders, one thing that these descriptive

[13]The non-binned scatterplots can be seen in Figure A.3. The conclusions are the same.

analyses do not do yet is control for ward-level heterogeneity, yearly, or seasonal maintenance trends. Regression analyses, described in the next section, address these concerns.

IV Method

The goal of this paper is to estimate effects of political incentives on road maintenance by comparing politically similar to politically dissimilar wards in the months immediately before the mayoral election. The exogenous timing of mayoral election years is used by comparing these differences across mayoral and non-mayoral election years. If there are differences in certain months, across political similarity and mayoral election years, then the most likely explanation is that political incentives are the reason for these differences.

The main approach is differences-in-differences and the specifications are as follows,

$$(1) \quad Maintenance_{myw} = \beta_1 IEP_{my} + \beta_2 PS_{yw} + \beta_3 PS_{yw} * IEP_{my} + \boldsymbol{\mu}_w + \boldsymbol{\phi}_y + \boldsymbol{\gamma}_m + e_{myw},$$

in which m stands for month, y stands for year, and w stands for ward. $\boldsymbol{\mu}_w$ is a vector of ward fixed effects to control for unobservable, time-invariant ward-level heterogeneity. $\boldsymbol{\phi}_y$ is a vector of year effects to control for variation in maintenance over time and $\boldsymbol{\gamma}_m$ is a vector of month fixed effects and seasonal maintenance patterns.

The treatment timing variable is IEP, which stands for inter-election period. IEP is a binary variable equaling 1 if it is a mayoral election year and the months of June-October. It must be a mayoral election year to see the heightened political incentives. It must be between June-October, because the mayor does not observe shocks to political similarity until after the May primary election.

The treated group variable is PS, which stands for politically similar. This variable is created from the within-ward deviation in registered Republican proportion of voters. PS is a dummy variable that equals 1 if within-ward deviation in Republican proportion of voters exceeds a particular cutoff value.[14] Like the descriptive analyses above, the baseline specification uses the median as the cutoff.

The coefficient of interest is β_3. It is the estimated effect of the interaction between politically similar wards and the inter-election period. It tests whether the mayor allocates maintenance differently to wards that are more or less similar when political incentives are heightened by impending elections.

The predicted effect of β_3 could be positive or negative. The political budget cycle literature suggests β_3 will be negative to maximize votes, because there are more votes to be gained in less similar wards (Rogoff, 1990).[15] The urban political economy literature suggests it could be positive, because road construction follows political motivations and local politicians leveraging local projects (Glaeser & Ponzetto, 2018; Glaeser & Shleifer, 2005).

IV.1 Identification

A benefit of this research design for understanding causal effects of political incentives is that it closely resembles differences-in-differences. This close resemblance makes the identifying assumption and how one can interpret the coefficients, if that assumption is true, easily understood. The identifying assumption is in the absence of the *mayoral* inter-election period, maintenance in politically similar and dis-similar wards would continue along parallel paths. If this is true, then β_3 can be considered an average treatment effect on the treated (ATT). Event study models cannot be straightforwardly adapted to examine this assumption, but the averages in the pre-election period of Table 2, finding no differences by mayoral election year and political similarity before the IEP begins, suggests this assumption is appropriate. State and national elections are not systematically related to the exogenous timing of mayoral elections and maintenance needs are unlikely to associate with state or national elections.

[14] Our preferred approach is to dichotimize this variable, because the econometric literature on differences-in-differences with continuous treatment is still developing (Callaway, Goodman-Bacon, & Sant'Anna, 2021) and the continuous binned scatterplots gave similar results to the dichotomized political similarity in the descriptive analyses.

[15] This is a straightforward implication of reducing unemployment.

IV.1.1 Possible Threats

One concern is that maintenance causes voters to move to other wards or register in response to prior maintenance projects; however, the research design leverages the exogenous timing of mayoral election years. For this endogenous registration to be an issue, one would have to believe that maintenance causes people to move to politically similar wards only in mayoral election years or people register differently in response to maintenance during mayoral election years. We believe that this is highly unlikely and no literature finds that people move or register in response to road maintenance.[16]

Another concern is that maintenance is highly seasonal. The reason that maintenance is seasonal is because it is less dangerous to do road work when it is not cold and icy. The identification strategy addresses this concern in two ways. First, month fixed effects ensure that estimated effects are within month, which means temperature and precipitation is held constant to the degree they vary by month. Second, the comparison group, politically dissimilar wards in the same city, have the exact same weather. It can be safely assumed that timing of mayoral elections is sufficiently exogenous that the cities ability to borrow or finance for maintenance makes the supply of maintenance sufficiently elastic.

Relation to Methods in Prior Work Using politically dissimilar wards as a comparison group for estimating the effect of political incentives is a deliberate design choice to deal with additional factors that have potential to confound the estimates such as per-unit maintenance costs and municipal budgets. Prior work identifies that the overall size of the budget can increase in election years, and this means that using a dummy variable approach with year dummies is likely inadequate in this context. Furthermore, road maintenance is different than road construction in that it is less intensive work and less likely to be planned long in advance.

[16] We do not claim that political similarity is randomly assigned and this is not required to estimate a causal effect. Random assignment or treatment ignorability would be required for an average treatment effect estimand, but this assumption is too strong in this context. Instead, this strategy should be seen as seeing using exogenous election cycle timing, like prior literature in this area, to investigate how political incentives affects areas that differ in their political similarity under a parallel trends assumption.

V Results

Table 3 presents estimates from Equation 1. Panel A investigates how maintenance changes in politically similar wards during all months in the inter-election period (June-October). Wards that are more politically similar receive 69.37 less meters of maintenance, but this result is imprecise.

One possible reason that the result in Panel A is statistically insignificant is because not all months of the IEP are equally close to the election. To examine whether there is no average effect due to heterogeneity in political incentives across months, Panel B redefienes the IEP as months that are closest to the election which is when the incentives to shift maintenance for political reasons might be greatest. As shown in Panel B, column 1, politically similar wards during October of mayoral election years receive 565.48 more meters of completed road maintenance than politically dissimilar wards. Using ward-clustered standard errors, this is statistically significant above 95% confidence. This coefficient is also economically significant, a 565 meter difference is 28.5% of a standard deviation in road maintenance. This is consistent with shifting maintenance away from places that are less likely to vote for the mayor, making it inconsistent with increasing the cost of voting for those in dissimilar wards. To see if the effect grows or shrinks, column 2 of Panel B includes August and September along with October in the IEP. The same sign is estimated, but the coefficient is smaller which leads to statistical insignificance.

The analysis suggests that more maintenance is completed in politically similar wards, which implies that road maintenance is seen more as an inconvenient disamenity. This could be an overly strong conclusion if that road maintenance is completed in different months. To investigate if shifts in the timing and not necessarily the amount of road maintenance is occurring, Panel C redefines the IEP as months furthest away from the election. In column 1 of Panel C, only June is included in the IEP and the estimate, statistically insignificant, is negative. In column 2 of Panel C, the IEP is expanded to include June, July, and August. The standard error shrinks with the additional observations and the point estimate changes from -150.94 to -605.76 which makes the estimate statistically significant above 99% confidence.

Poisson Model So far, the results have depended on model type, OLS. Depending on OLS could be problematic if there are many 0's or outliers in the maintenance data, so Table A.2 re-estimates the coefficients using Poisson regression and count of heavy projects as the dependent variable.[17] Just like the OLS estimates, only Panel B, column 1 and Panel C, column 2 are statistically significant above at least 5% confidence which means the results are not dependent on estimation procedure.[18]

Composition of Types of Maintenance Since elected officials can also shift composition, instead of just total amounts, of spending (Drazen & Eslava, 2010), whether political incentives affect different types of maintenance differently is examined. These results are presented in Figure 5. The coefficients all go in the same direction as the aggregated measure used previously, meaning there is no evidence that the composition of maintenance changes.[19]

V.1 Randomization Inference

To this point, the standard errors have been calculated using clustered standard errors at the ward level. There are only 32 wards in the city of Pittsburgh. The appropriateness of clustered standard errors relies on having a large number of clusters, and it could be possible that statistical significance is purely a function of this possibly violated assumption. To do inference that is not vulnerable to violations of this assumption, randomization inference is used which comes from Fisher (1935) and is similar to Cunningham and Shah (2018) and Buchmueller, DiNardo, and Valletta (2011). This procedure randomizes the political similarity within-wards across years which randomly assigns treatment, estimates the regression, collects the t-statistics for the coefficient, and then repeats this 1,000 times. This procedure does not assume a large number of clusters like clustered standard errors do.

[17] Poisson consistently estimates parameters when the conditional mean is specified correctly (J. M. Wooldridge, 1999). Furthermore, J. Wooldridge (2021) shows that differences-in-differences is appropriate with non-linear estimators such as Poisson and simulations suggest OLS performs poorly in terms of efficiency with count dependent variables. Finally, Poisson is more appropriate than Negative Binomial because Guimaraes (2008) shows that it only controls for ward fixed effects under a spefific set of assumptions and Allison and Waterman (2002) shows that Negative Binomial is not a true panel model due to it allowing for individual-specific regressors.

[18] The randomization inference procedure is depicted graphically in Figure A.4 and the Poisson coefficients retain the same level of statistical significance as those in the table.

[19] We use further disaggregated categories in Figure A.5 and Figure A.6, but there are not enough projects for reliable inference in many cases. Similar conclusions emerge that composition of maintenance is unaffected by political incentives.

The randomization inference p-value comes from comparing how many times the t-statistics were farther from 0 in the randomized sample to the actual t-statistic and these are presented in brackets under the standard errors in Table 3. In Panel B, column 1 the estimate is statistically significant above 95% confidence with both the clustering approach and with the randomization inference procedure. The bracketed p-value means that a more extreme t-statistic was observed in only 26 out of 1,000 of the falsely randomized treatments. The estimate in Panel C, column 2 also retains its original statistical significance above 99% confidence. The statistical significance is not sensitive to relaxing the many clusters assumption.

V.2 Car Crashes

Since road maintenance is shifted, it is possible that other road outcomes that depend on maintenance are also shifted such as car crashes. Road maintenance has ambiguous effects on car crashes; smoother roads lead to fewer crashes (Bock et al., 2021), but maintenance also can reduce available lanes, increasing traffic congestion which potentially increases crashes. Data on car crashes come from the Pennsylvania Department of Transportation. All crashes that do not occur on Pittsburgh-owned local roads are excluded. Poisson models are used since the dependent variables are count variables.

The results are presented in Table 4. For convenience, the same explanatory variable's sign on maintenance is summarized at the top. Panel A investigates how political incentives affect the number of crashes that occur on local roads. In column 4 of Panel A, which represents June-August and less maintenance occurring, there is a statistically significant reduction in the number of car crashes occurring. While 5 coefficients were estimated, the only statistically significant coefficient is the one of two where a statistically significant coefficient on road maintenance was also found.

The coefficient in Panel A, column 4 is statistically significant at 95% confidence and is a 9.4% reduction in car crashes from a mean of 7.565 crashes. This translates into a reduction of about 0.711 vehicular crashes. While rough roads reduce road safety (Bock et al., 2021), when maintenance that smooths roads is more dense than usual, it can also make roads contemporaneously more dangerous. Without further information, it could be hard to decipher whether this is economically meaningful.

One way to shed additional light on whether 0.711 less vehicular crashes is economically meaningful is to use other crash-related dependent variables. Along these lines, Panel B uses count of major injuries as the dependent variable instead. In Panel B, column 1 there is a coefficient that is statistically significant at 90% confidence, which happens to be in the same month in which political incentives are estimated to increase maintenance. The coefficient's magnitude is large, meaning there is a 166.4% increase in major injuries in car crashes on roads with more maintenance. One reason this coefficient implies a large increase is due to the low average number of major injuries in car crashes of 0.16. Combining the coefficient and average number of monthly car crash major injuries, this coefficient means that political incentives to shift road maintenance are responsible for an estimated 0.266 additional major injuries from car crashes.[20]

Finally, trucks have been found to be deadly in many contexts (Anderson & Auffhammer, 2014; Muehlenbachs et al., 2021) and could be especially accident-prone if there are lane closures and additional congestion from maintenance. To investigate this, Panel C uses heavy truck crashes as the dependent variable.[21] There are two statistically significant coefficients in Panel C, columns 3 and 4. The coefficient in column 4, representing the same months in which political incentives reduce road maintenance, is statistically significant at 95% confidence and suggests that less maintenance reduces the number of heavy truck crashes. The coefficient is also large suggesting that the reduction in maintenance of 565 meters of maintenance reduces heavy truck crashes by 103.3%. The mean of monthly heavy truck accidents is 0.215, implying that political incentives for road maintenance are responsible for an additional 0.222 heavy truck accidents in politically dissimilar wards.[22]

VI Conclusion

Research consistently shows that elected officials predictably manipulate the economy via fiscal policy to maximize their chances for winning elections (Rogoff, 1990; Veiga & Veiga, 2007). Traditionally, this means election year spending to reduce unemployment; however, if that spend-

[20]There is also no evidence that this is due to a reduction in fatalities or less severe injuries.

[21]A heavy truck is defined in the data as a single vehicle or tractor-trailer combination designed for carrying a heavy load of property on or in the vehicle. Includes: single unit trucks (e.g., coal truck), tractor-trailers, motor homes, etc.

[22]There are no statistically significant effects in the entire IEP on maintenance, but it is negative which makes the coefficient in column 3 consistent with the results so far.

ing is spent on road construction or maintenance, then it could be a disamenity to local residents (Glaeser & Ponzetto, 2018). Furthermore, it's not obvious that road maintenance and construction are viewed similarly because politicians have differing incentives for them (Schmitt, 2015). Political incentives are consistently identified as concerns in the total spending on roads (Veiga & Veiga, 2007) and to shift the composition of spending (Drazen & Eslava, 2010). It's important to further understand this because maintenance and construction have differing effects on the macroeconomy (Kalaitzidakis & Kalyvitis, 2004; Kalyvitis & Vella, 2015; Rioja, 2003) and heterogeneous impacts with distributional concerns (Brueckner & Selod, 2006; Gibson & Rioja, 2017).

This paper estimates the effect of political incentives on road maintenance timing using a differences-in-differences framework which addresses other major determinants of road maintenance timing, including municipal budgets and seasonality. It finds that the timing of road maintenance is shifted according to the political similarity of areas in mayoral election years. This shift leads to two negative additional consequences: additional car crashes and increased costs of road maintenance. As described in Section B, these estimates imply the lower bound of the cost of local political incentives for road maintenance is calculated to be \$271,869 in Pittsburgh over the three mayoral elections in the sample. When these costs are extended across medium-large cities over the past sixty years, political incentives due to mayoral election cycles affecting maintenance are calculated to cost \$421.4 million.

Despite filling an important gap in the literature, there are several limitations. First, the data is for only one city which may limit the external validity. Another limitation is the start date for maintenance projects is not observed which would help determine if the time it takes to complete projects is somehow changed due to political incentives such as Lehne et al. (2018) in which the construction never even occurs. Finally, these results cannot be compared and contrasted with road construction since it is unobserved.

Political incentives alter decision-making in a wide variety of contexts. This implies that a policy fix could be more market-oriented solutions; but, goods with large positive externalities might not be funded to a socially optimal level. One solution that has been proposed, a hybrid of political and market solutions, is to hire a civil asset manager who is in charge of managing city assets (Detter & Fölster, 2017). It is likely that less organized political groups are harmed by manipulation of road maintenance for political reasons similar to road construction (Glaeser & Ponzetto, 2018).

This paper does not suggest that maintenance should not be completed; maintenance reduces road roughness, which is good for road safety as shown by Bock et al. (2021). This paper suggests that maintenance should not be shifted to the extent that it is according to political incentives.

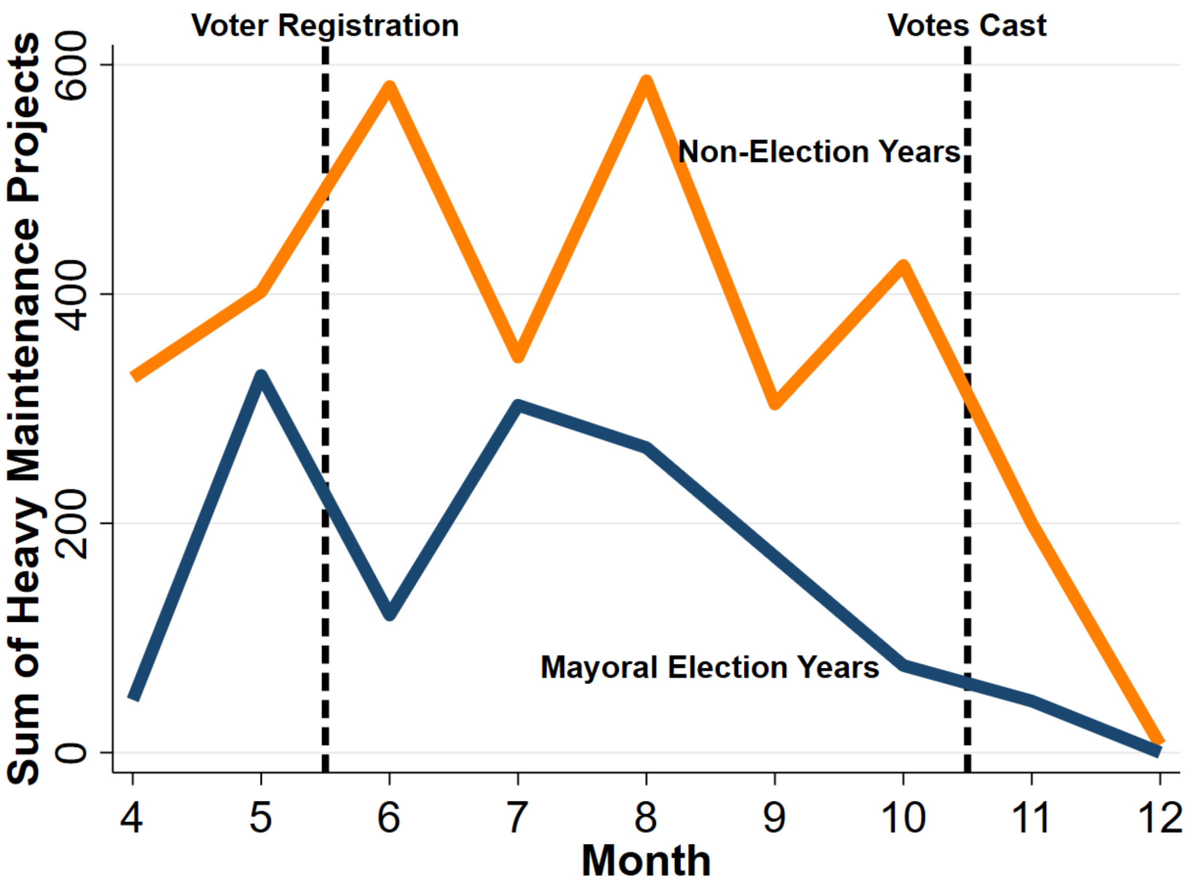
Finally, future research can build on this work in a number of ways. It would be interesting to find other disaggregated data that might be manipulated during mayoral election years. Understanding how voters respond to maintenance in election polls would also be helpful in building a holistic roadmap of how politicians and the electorate view road maintenance and construction.

TABLE 1: TYPES OF MAINTENANCE PROJECTS

Type of Maintenance	Description	Classification
Heavy		
Base Repair	Removing damaged pavement, determining cause of failure and appropriate solution, resurfacing of road	Heavy
SuperPave	Pavement mix designed to combat deformation and low temperature cracking	Heavy
Mill and Overlay (SuperPave)	Existing pavement removed, milled, surface overlaid with SuperPave pavement	Heavy
Mill and Overlay, $\geq$ 3”	Existing pavement surface removed, milled to a depth of greater than 3 inches, surface overlaid with new asphalt pavement	Heavy
Non-Heavy		
Mill and Overlay, $<$ 3”	Existing pavement surface is removed, milled to depth of less than 3 inches, surface is overlaid with new asphalt pavement	Non-Heavy
AC Overlay	Pavement is placed down on top of existing pavement, no milling of existing road	Non-Heavy
Mechanical Patching	Small areas and irregularities patched over	Non-Heavy
Profile Milling	Just the edges of the road are milled down	Non-Heavy

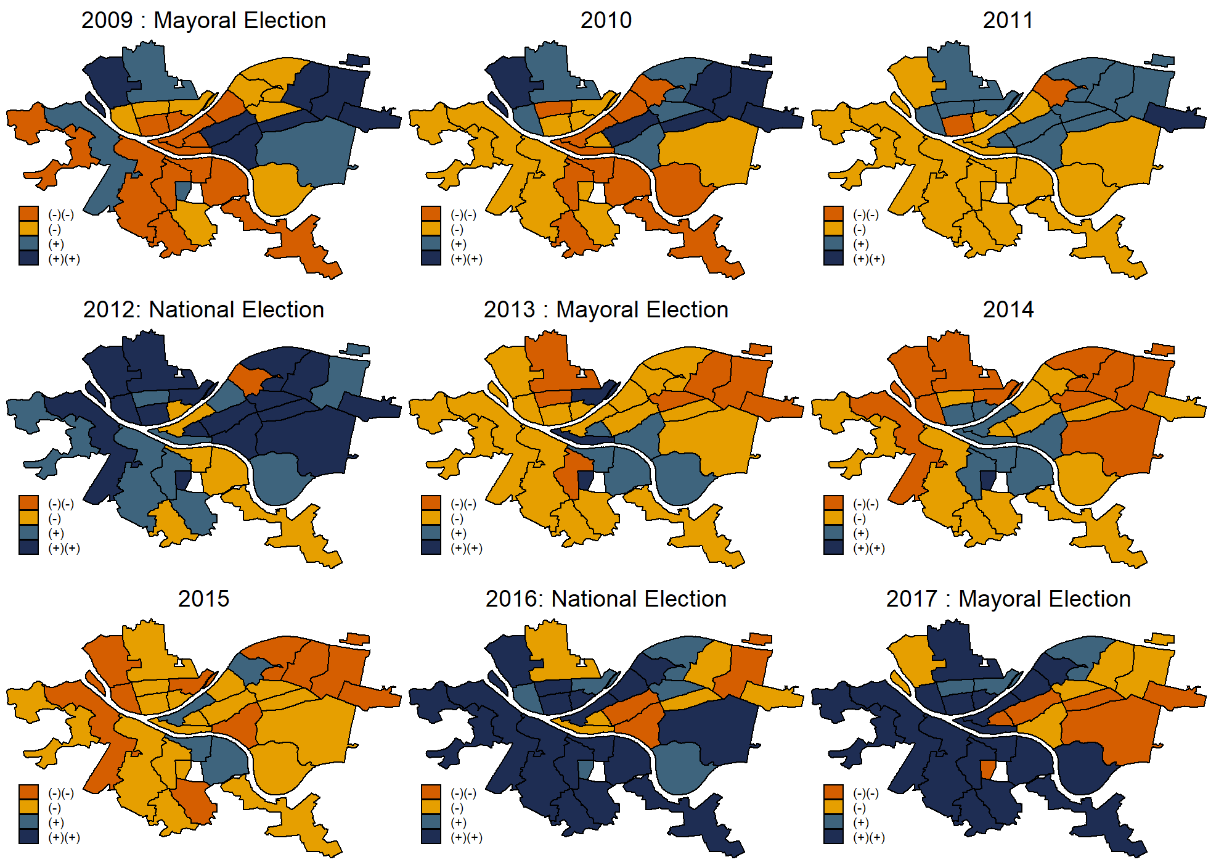
Note: Types of maintenance projects completed in Pittsburgh are shown above. “Heavy” projects are projects where at least 3 inches of road are removed and replaced. Table A.1 shows counts of each type of maintenance at the ward-month-year level. AC = Asphalt concrete.

FIGURE 1: MONTHLY COMPLETION OF HEAVY ROAD MAINTENANCE



Note: Graph displays sum of heavy paving projects per year in the city of Pittsburgh from 2009-2017. The red line represents years when there is a mayoral election. The blue line represents years that do not have a mayoral election. Mayoral elections are held in 2009, 2013, and 2017. Numbers represent raw project, not adjusted to yearly averages. Red-dashed lines mark the length of the inter-election period. Non-election years refers to non-mayoral election years, thus it includes 2012 and 2016 which are national election years.

FIGURE 2: WITHIN-WARD POLITICAL SIMILARITY DEVIATION OVER TIME AND SPACE



Note: Within-ward political similarity deviations across Pittsburgh wards for all years of data (2009-2017). Mayoral election years are on the diagonal. The proportion of registered Democratic voters is used to proxy for political similarity. Deviations in similarity within a ward are measured as 0.675 standard deviation changes from that same ward's median political similarity measure. If a ward became "less politically similar" (more registered Republicans) relative to its average similarity across the sample time period, it is shown in orange above as a negative change (-); "more politically similar" (less registered Republicans) is shown in navy above as a positive change (+). Between 0 and 0.675 standard deviations from the median are shown in lighter shades of these respective colors (Small negative change: (-); small positive change: (+)) Greater than 0.675 standard deviations from the median in either direction are represented by darker shades of their respective colors (Large negative change: (-)(-); large positive change: (+)(+)).

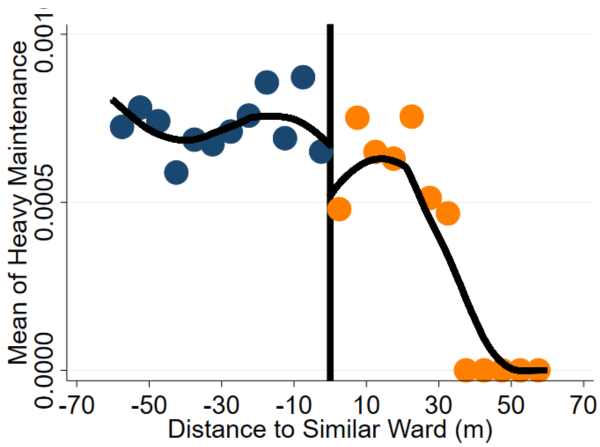
TABLE 2: NUMBER OF HEAVY MAINTENANCE PROJECTS COMPLETED BY ELECTION YEAR, MONTH, AND POLITICAL SIMILARITY

	Mayoral Election Year				
	Non-Election Year		**Election Year**		**Row T-Test**
	Maintenance Mean	SE	Maintenance Mean	SE	Difference
Before Inter-Election Period					
April-Dissimilar (n=144)	1.45	(0.54)	0.31	(0.19)	1.14
April-Similar (n=144)	1.96	(0.52)	0.65	(0.29)	1.31*
May-Dissimilar (n=144)	2.34	(0.58)	4.35	(1.21)	-2.01*
May-Similar (n=144)	1.84	(0.47)	2.50	(0.77)	-0.66
Inter-Election Period					
June-Dissimilar (n=144)	2.44	(0.80)	1.52	(0.90)	0.92
June-Similar (n=144)	3.61	(0.80)	0.98	(0.31)	2.64**
July-Dissimilar (n=144)	1.47	(0.42)	4.31	(1.01)	-2.84***
July-Similar (n=144)	2.13	(0.44)	2.00	(0.74)	0.13
August-Dissimilar (n=144)	3.41	(0.67)	3.77	(0.81)	-0.36
August-Similar (n=144)	2.70	(0.67)	1.77	(0.52)	0.93
September-Dissimilar (n=144)	0.67	(0.22)	0.98	(0.40)	-0.31
September-Similar (n=144)	2.50	(0.45)	2.58	(0.87)	-0.08
October-Dissimilar (n=144)	2.93	(0.73)	0.38	(0.19)	2.55**
October-Similar (n=144)	1.50	(0.38)	1.21	(0.39)	0.29
After Inter-Election Period					
November-Dissimilar (n=144)	1.29	(0.33)	0.56	(0.28)	0.73
November-Similar (n=144)	0.79	(0.24)	0.38	(0.14)	0.42
December-Dissimilar (n=120)	0.06	(0.05)	0.00	(0.00)	0.06
December-Similar (n=136)	0.01	(0.01)	0.00	(0.00)	0.01

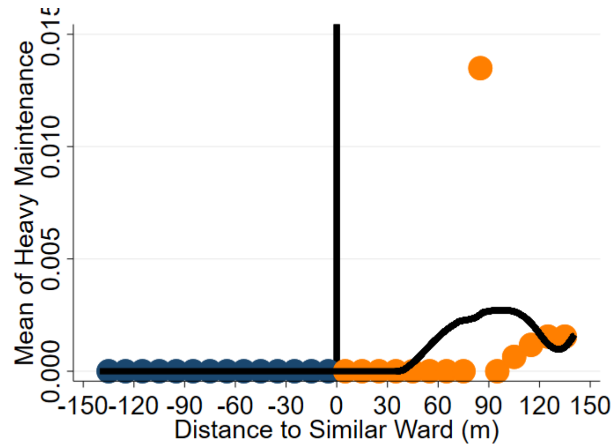
Note: The mean count of heavy maintenance projects per ward in each month-similarity classification is shown, separated by whether it is a mayoral election year (2009, 2013, 2017) or not. Descriptions of heavy projects are shown in Table 1. There are three election years and six non-election years in the data. The mean count of heavy maintenance over the total nine years is also shown. Similarity classification is determined by within-ward deviations in political similarity in a given year. N is the number of wards that fall into each month-similarity classification over the length of the data (2009-2017). SE = standard error. Row T-test conducted to compare election year completed road maintenance to non-election year completed road maintenance in each month. For the T-test column: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

FIGURE 3: HOW BEING IN A DISSIMILAR COMPARED TO A SIMILAR WARD AFFECTS MAINTENANCE BY MAYORAL ELECTION YEAR AND TIME TO ELECTION

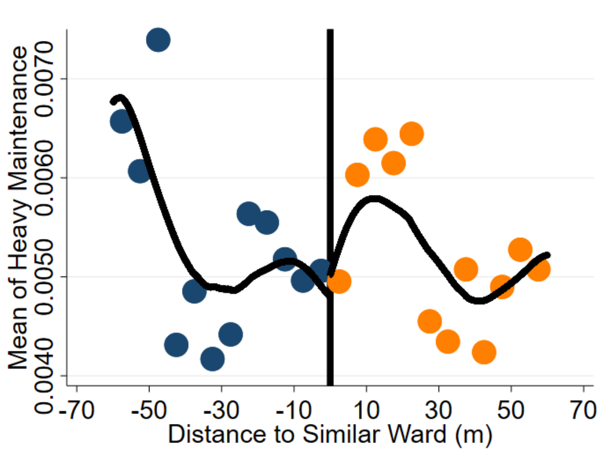
(A) MAYORAL ELECTIONS, JUNE-AUGUST



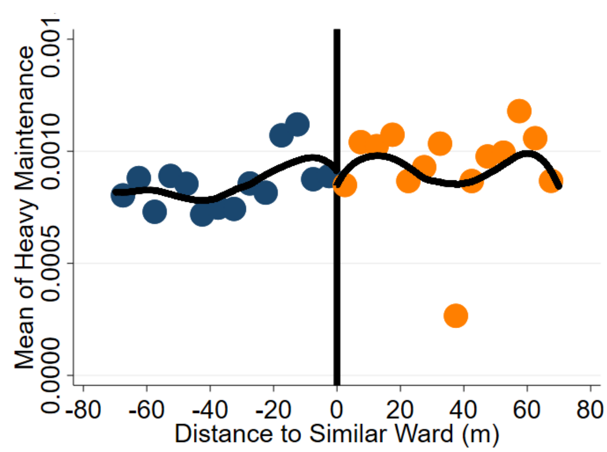
(B) MAYORAL ELECTIONS, OCTOBER



(C) NON-MAYORAL ELECTIONS, JUNE-AUGUST



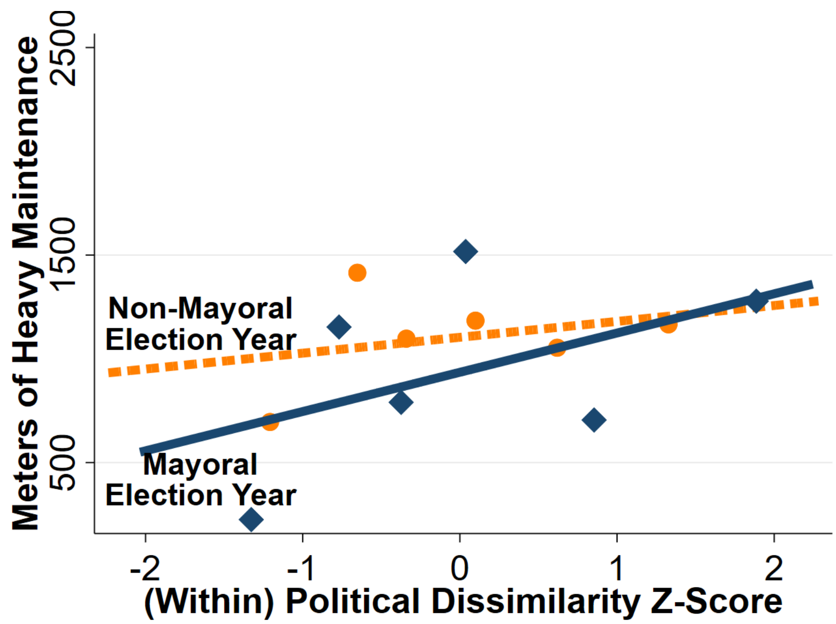
(D) NON-MAYORAL ELECTIONS, OCTOBER



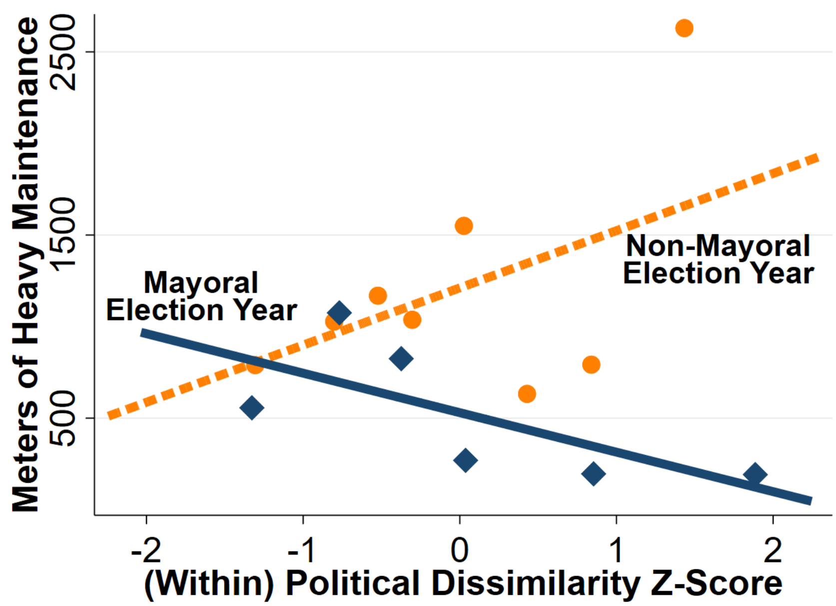
Note: Unit of analysis is the 1 meter road segment in a month-year. Plots show the average, within distance from similarity change border, of a road being maintained. Bins with positive values are in dissimilar wards. Roads within 10 meters of the boundary are dropped since these roads are used as ward boundaries. Fitted lines are locally-weighted sum of squares (lowess). In (A), the three mayoral election years and June-August are shown. In (B), the three mayoral election years and October are shown. In (C), the 6 non-mayoral election years and June-August are shown. In (D), the 6 non-mayoral election years and October are shown.

FIGURE 4: BINNED SCATTERPLOTS OF HEAVY ROAD MAINTENANCE BY MAYOR-VOTER PARTY SIMILARITY, ELECTION YEAR, AND SEASON

(A) SUMMER, JUNE-AUGUST



(B) FALL, OCTOBER



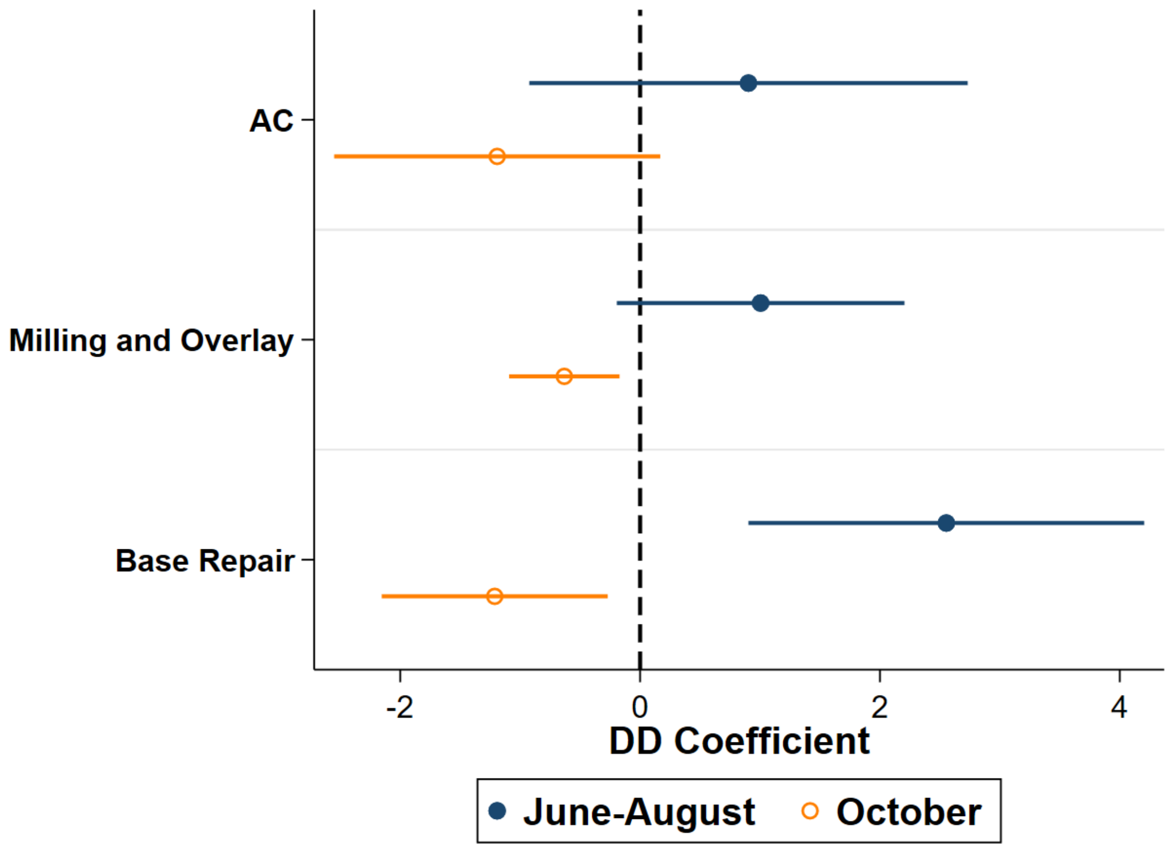
Note: N = 288 ward-year observations (144 observations each for election and non-election years) optimally binned scatter plots using Cattaneo et al. (2019). Dependent variable is meters of road maintained. Independent variable is within-ward political similarity deviations from average within-ward political similarity. Orange points and lines represent non-mayoral election years and navy points and lines represent mayoral election years.

TABLE 3: THE EFFECT OF POLITICAL INCENTIVES ON ROAD MAINTENANCE

Panel A: Entire IEP		
	(1)	
Politically Similar * IEP (June-October)	-69.37	
	(161.45)	
	[0.704]	
Observations	2560	
Treated Observations	240	
Panel B: Fall, Near Election Time		
	(1)	(2)
Politically Similar * IEP (October)	565.48**	
	(254.61)	
	[0.026]	
Politically Similar * IEP (August-October)		323.81
		(213.18)
		[0.127]
Observations	2560	2560
Treated Observations	48	144
Panel C: Summer, Further from Election		
	(1)	(2)
Politically Similar * IEP (June)	-150.94	
	(345.44)	
	[0.795]	
Politically Similar * IEP (June-August)		-605.76***
		(195.40)
		[0.004]
Observations	2560	2560
Treated Observations	48	144

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Model is shown in Equation 1. Outcome is meters of road maintenance. Ward, month, and year (uninteracted) fixed effects are included in every column and panel. Standard errors, clustered at the ward level, are shown in parentheses. P-values from the randomization inference are shown in brackets. Coefficients in every panel and column are the DD coefficient of interest ("PSxIEP"). Panel A include the entire time between the primary and general election (June - October), or the inter-election period (IEP). Panel B isolates months closer to the election, while Panel C looks at months further from the election.

FIGURE 5: DIFFERENCE IN DIFFERENCE COEFFICIENTS BY MAINTENANCE TYPE



Note: Figure illustrates the DD coefficient of interest estimated from Equation 1, disaggregated by maintenance type. Different colors represent different times of the inter-election period (IEP). Navy represents June-August and orange represents just October. AC = Asphalt concrete.

TABLE 4: HOW POLITICAL ROAD MAINTENANCE CYCLES AFFECT CAR CRASHES

	More Maintenance		Entire IEP	Less Maintenance	
	(1)	(2)	(3)	(4)	(5)
	Oct	Aug-Oct	Jun-Oct	Jun-Aug	Jun
Panel A: Crashes					
IEP X PS	0.02	0.02	-0.04	-0.09**	-0.02
	(0.07)	(0.05)	(0.04)	(0.04)	(0.08)
Average Crashes					7.565
Treatment Month(s) Average Crashes	7.896	7.840	7.604	7.410	7.646
Panel B: Major Injuries					
IEP X PS	0.98*	0.37	0.19	-0.19	0.12
	(0.51)	(0.38)	(0.27)	(0.39)	(0.53)
Average Major Injuries					0.160
Treatmont Month(s) Average Major Injuries	0.250	0.181	0.171	0.146	0.167
Panel C: Heavy Truck Crashes					
IEP X PS	-0.12	-0.09	-0.34*	-0.71**	-0.26
	(0.58)	(0.24)	(0.19)	(0.28)	(0.55)
Average Average Truck Accidents					0.215
Treatment Month(s) Average Truck Accidents	0.170	0.218	0.198	0.162	0.146

Note: Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Estimates are from Poisson conditional fixed effect models. Ward, month, and year (uninteracted) fixed effects are included in every column. Standard errors, clustered at the ward level, are shown in parentheses. DD coefficient of interest is "IEPxPA." Panel A-D change the dependent variables of interest from heavy maintenance count to various car accident outcomes. Only car crashes from 2009-2017 that occur on Pittsburgh-owned roads are included in every column.

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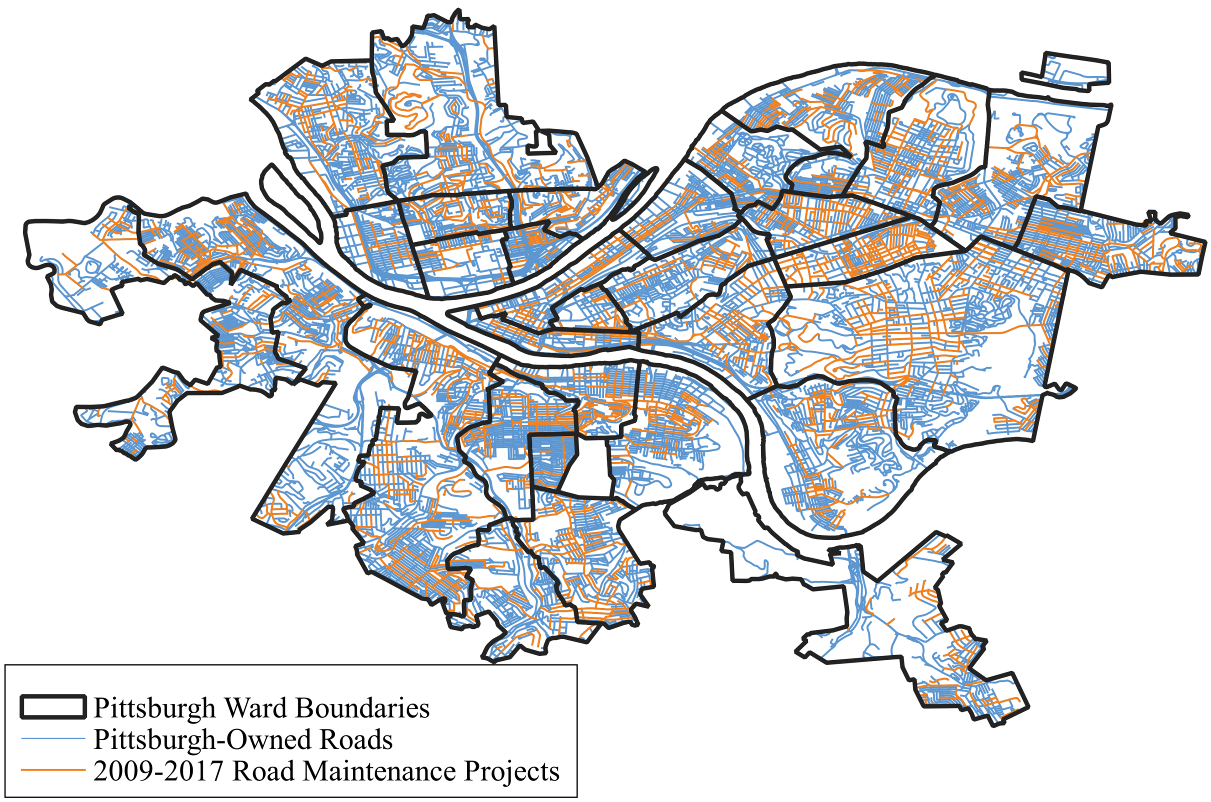
A Additional Tables and Figures

FIGURE A.1: MAINTENANCE PROJECTS AND ROADS SUPERIMPOSED ON POLITICAL WARDS



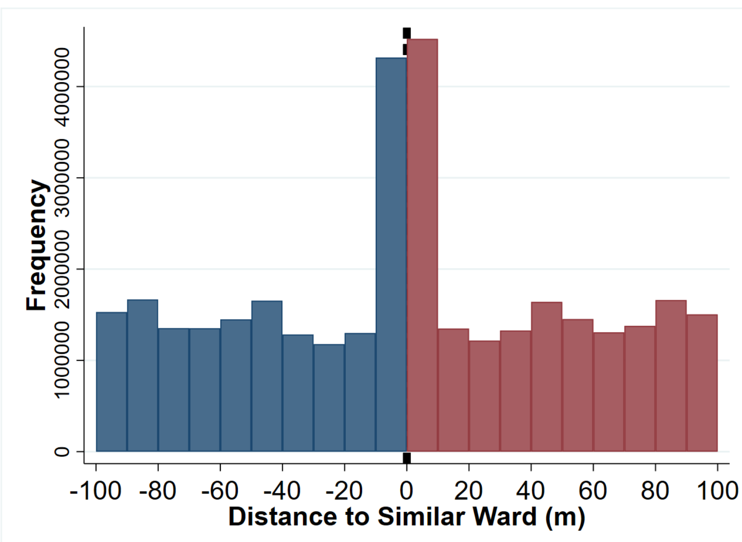
Note: This map of Pittsburgh shows the total universe of city-maintained roads and where maintenance occurred during the sample period. The blue lines are all city-maintained roads, and the orange lines show segments where maintenance occurred. Thick black lines denote ward boundaries.

TABLE A.1: DESCRIPTIVE STATISTICS

	Mean	Median	SD	Min	Max	Count
Registered Republican Voters (%)	13.17	13.08	5.31	2.9	26.3	2592
Within Ward Deviation of Republican Voter (%)	-0.00	-0.15	0.94	-2.3	2.3	2592
Political Similar Above Median	0.50	0.50	0.50	0.0	1.0	2592
Mayoral Election Year	0.33	0.00	0.47	0.0	1.0	2592
All Road Maintenance Projects	2.30	0.00	5.73	0.0	66.0	2592
Heavy Road Maintenance	1.91	0.00	5.23	0.0	66.0	2592
Light Road Maintenance	0.38	0.00	1.79	0.0	25.0	2592
AC Overlay <= 2 in.	0.03	0.00	0.40	0.0	11.0	2592
AC Overlay > 2 in.	0.19	0.00	1.25	0.0	22.0	2592
Milling and Overlay Projects	1.63	0.00	4.59	0.0	49.0	2592
Base Repair	0.23	0.00	1.19	0.0	17.0	2592
Length of Maintenance (in meters)	786.45	0.00	1982.97	0.0	24421.2	2592
Local Road Vehicle Accidents	7.58	7.00	5.47	0.0	43.0	2592
Potholes from 311 Calls	21.15	14.00	21.83	1.0	176.0	843

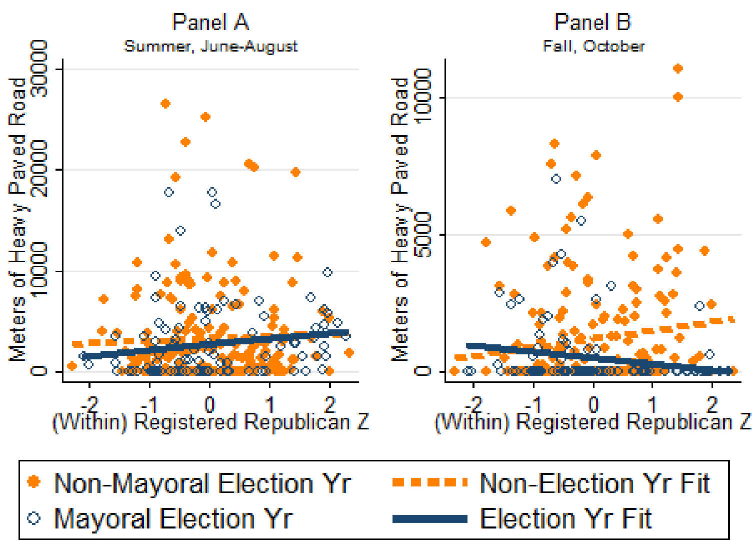
Note: Table shows descriptive statistics for the ward-month-year observations. AC = Asphalt concrete.

FIGURE A.2: NUMBER OF ROAD SEGMENTS BY DISTANCE TO WARD BOUNDARY



Note: Figure illustrates the frequency of 1 meter road segments by distance to ward boundaries. 1 segment per year kept, because political similarity changes yearly.

FIGURE A.3: METERS OF MAINTENANCE BY WITHIN-WARD PARTY SIMILARITY DEVIATION Z-SCORE, UNBINNED



Note: n = 288 ward-year observations (144 observations each for election and non-election years). Dependent variable is meters of road maintained. Independent variable is within-ward registered Republican deviations from within-ward average (Republican Z). Only heavy maintenance included. Legend consistent for all panels. Orange points represent meters of completed maintenance at each Z-score in non-mayoral election years, while blue points represent meters of completed maintenance at each Z-score in mayoral election years. The dashed-orange line is the line of best fit for non-election years and the solid blue line is the line of best fit for election years.

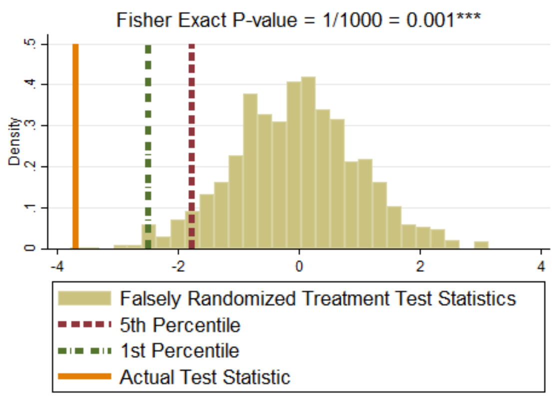
TABLE A.2: THE EFFECT OF POLITICAL INCENTIVES ON ROAD MAINTENANCE, POISSON ESTIMATES

Panel A: Entire IEP		
	(1)	
Politically Similar * IEP (June-October)	-0.22	
	(0.20)	
Observations	2560	
Treated Observations	240	
Panel B: Fall, Near Election Time		
	(1)	(2)
Politically Similar * IEP (October)	1.30***	
	(0.50)	
Politically Similar * IEP (August-October)		0.22
		(0.25)
Observations	2560	2560
Treated Observations	48	144
Panel C: Summer, Further from Election		
	(1)	(2)
Politically Similar * IEP (June)	-0.35	
	(0.62)	
Politically Similar * IEP (June-August)		-0.78***
		(0.21)
Observations	2560	2560
Treated Observations	48	144

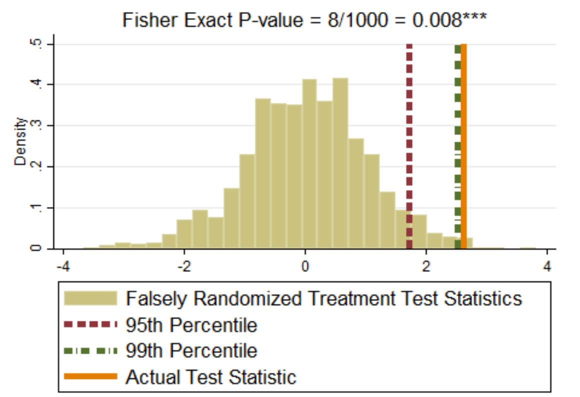
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Model is shown in Equation 1. Estimates are from Poisson conditional fixed effect models. Outcome is count of road maintenance projects. Ward, month, and year (uninteracted) fixed effects are included in every column and panel. Standard errors, clustered at the ward level, are shown in parentheses. Coefficients in every panel and column are the DD coefficient of interest ("PSxIEP"). Panel A include the entire time between the primary and general election (June - October), or the inter-election period (IEP). Panel B isolates months closer to the election, while Panel C looks at months further from the election.

FIGURE A.4: RANDOMIZATION INFERENCE FOR POISSON

PANEL A: JUNE-AUGUST DD COEFFICIENT PLACEBO Z-STATISTICS DISTRIBUTION

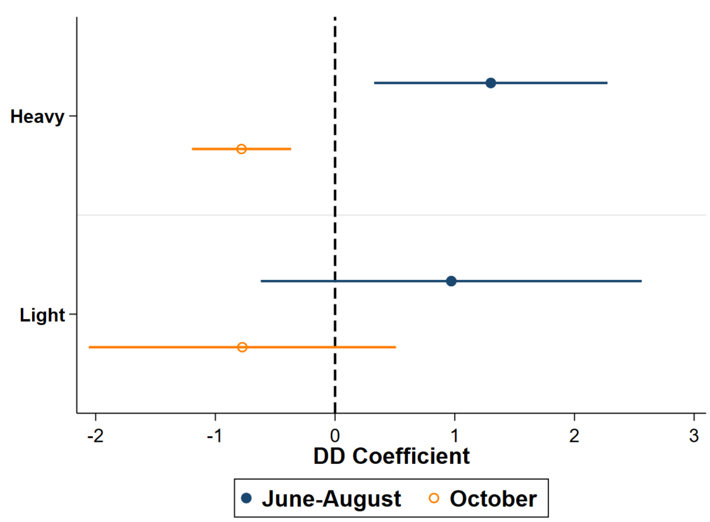


PANEL B: OCTOBER DD COEFFICIENT PLACEBO Z-STATISTICS DISTRIBUTION



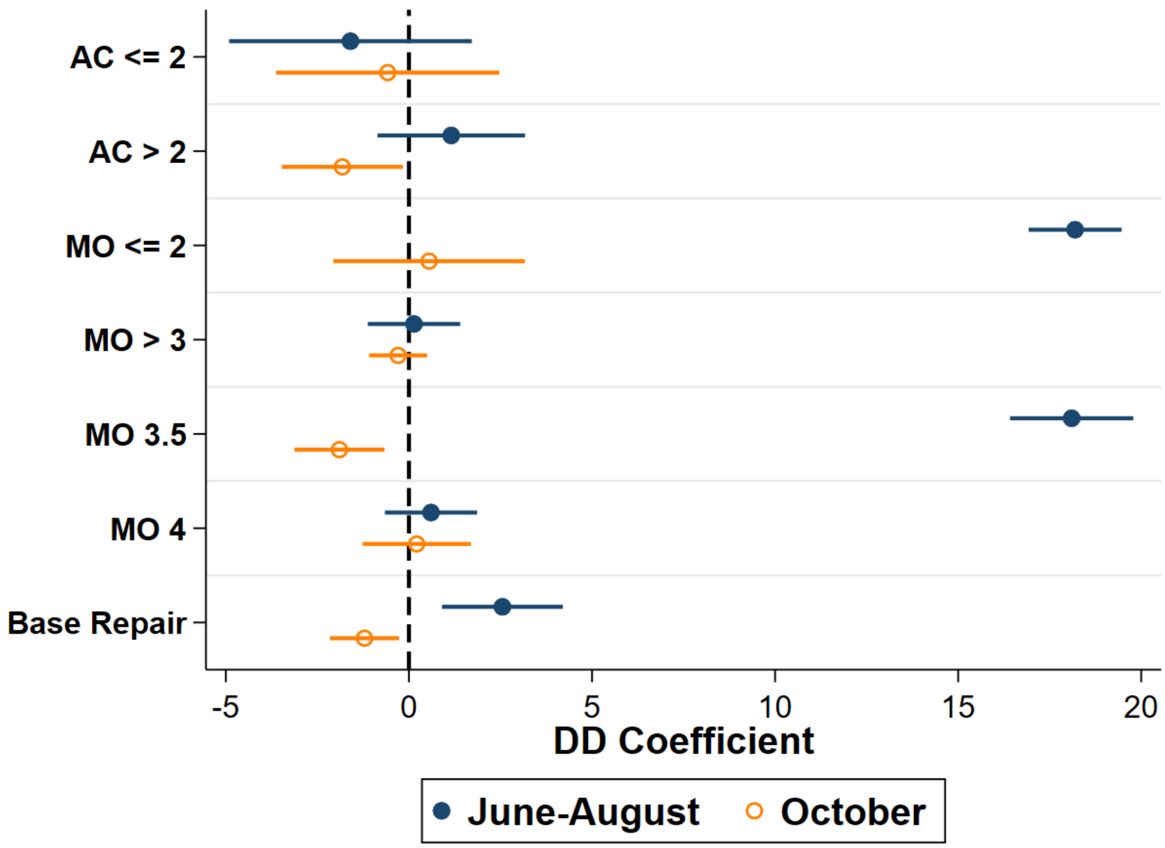
Note: Political similarity is shuffled within wards, across years. This leaves the distribution of similarity within-ward the same. Panel A: The test statistics displayed are from estimating PSxIEP in the Panel A, column 2 of Table A.2 under falsely randomly assigned political similarity within ward across years. The actual test statistic is lower than the falsely randomized test statistics in 999/1000 permutations. In Panel B: The test statistics displayed are from estimating PSxIEP in Panel B, column 3 of Table A.2 under falsely randomly assigned political similarity within ward across years. The actual test statistic is greater than the falsely randomized test statistics in 992/1000 permutations.

FIGURE A.5: DIFFERENCE IN DIFFERENCE COEFFICIENTS BY MAINTENANCE TYPE



Note: Figure illustrates the DD coefficient of interest estimated from Equation 1, disaggregated by broad maintenance type. Different colors represent different times of the inter-election period (IEP). Navy represents June-August and orange represents just October. See Figure 5 for full disaggregation.

FIGURE A.6: DIFFERENCE IN DIFFERENCE COEFFICIENTS BY MAINTENANCE TYPE



Note: Figure illustrates the DD coefficient of interest estimated from Equation 1, disaggregated by maintenance type. Different colors represent different times of the inter-election period (IEP). Navy represents June-August and orange represents just October. AC = Asphalt concrete. MO = Milling and Overlay.

B Costs of Local Election Road Maintenance Shifts

The mayoral election shifts maintenance across time which is sub-optimal since maintenance delays increase costs (Burningham & Stankevich, 2005). Furthermore, these shifts in maintenance timing lead to car crashes and major injuries. In Table B.1, the estimates from Tables 3 and 4 are used, along with supplemental information, to summarize how costly maintenance shifts and the resulting additional car crashes are.

B.1 Costs Due to Delayed Maintenance

Panels A-C of Table B.1 address how maintenance shifts increase maintenance costs. Table 3 estimates that of the 605.76 less meters of road maintenance are completed in June-August and 565.48 more meters are completed in October which is where the figures in Panel A come from. This implies that 40.28 meters of road maintenance are delayed indefinitely.

Next, calculating the total costs requires a price to go along with the quantities in Panel A. These prices are summarized in Panel B of Table B.1.[23] Dividing total expenditure on maintenance by total meters maintained from 2009-2017 in Pittsburgh, the average cost per meter is calculated to be $45.55.[24] This per meter cost only applies to maintenance that is optimally scheduled and not to delayed maintenance.

To map out the relationship between maintenance time delay and multiple more expensive, data points from Burningham and Stankevich (2005) are used. These data points are that a 3 year delay causes maintenance to be 6 times more expensive and a 5 year delay causes maintenance to be 18 times more expensive. A third data point is that a 0 year delay maintenance causes no additional expense by definition, so the point (0 years, 1 times as expensive) is added. An exponential curve fits these data points well with an R-squared of 0.9995. Furthermore, exponential growth in cost multiples makes sense in this context since not repairing surface cracks in a timely fashion could

[23]Total expenditure on road maintenance comes from the Pittsburgh Comprehensive Annual Financial Reports (https://pittsburghpa.gov/controller/cafr) and Pittsburgh Capital Budgets data (https://pittsburghpa.gov/council/capital-budgets).

[24]We clearly recognize that not all maintenance costs the same, but the expenditure data is not broken down by type of maintenance, so further disaggregation is impossible. For each year, the per meter average cost is calculated by taking the total paving expenditures in Figure B.2 and dividing by the total length of maintenance. Then, the average across years is calculated, weighted by the expenditure amount in each year. This is done to account for years that happened to have larger amounts of spending on paving. Not weighting like this does not change the average per meter cost by more than $1.92 per meter.

lead to greater structural deficiencies which are more expensive to address. The equation of this fitted exponential curve is: (1.0153 * (exp(0.5796 * delayed years))).

In order to calculate the costs from the quantity shifts in Panel A and the prices from Panel B, assumptions must be made about how many years the shifts are delayed. For the 565.48 meters that are delayed from June-August until October, the possible range of years is 3/12 to 5/12. This difference is split, and it is assumed that those 565.48 meters of roads are maintained 4/12 or 0.33 years later. This is shown in the first line of Panel C.

It is less straightforward to assign a delay time to the leftover 40.28 meters. It is unlikely that they are maintained directly after the November election, because the weather is not conducive to road maintenance and few road maintenance projects occur in the winter months from November to March. This means that a road that should have been maintained in June of year y may not be maintained until April of year $y + 1$, implying a delay of at least 11/12 years. While this is the minimum, the maximum of years of delay is unbounded for these 40.28 meters. To gain tractability, we assume that those roads will be maintained by at least the next mayoral election year, 4 years later. This difference could be split as well, but conservatively we assign a value of a delay of 2 years.

The summary of how much additional costs comes from maintenance shifts is shown in the first line of Panel E. Deviating from the optimal paving schedule makes paving \$37,662 more expensive per mayoral election in Pittsburgh. This number comes from simple addition and multiplication,

$$TC = Delay\ Cost + Leftover\ Cost, \tag{2}$$

in which each term is calculated,

$$C = Length * Per\ Meter\ Cost * Delay\ Multiplier. \tag{3}$$

To put this in perspective, an expenditure of \$37,662 would pay for 826.84 meters of optimally scheduled road maintenance opposed to just 605.76 meters.

B.2 Costs Due to Major Injuries in Car Crashes

Next, there are additional costs of shifting maintenance due to how it increases car crashes and major injuries. The Poisson coefficient, 0.98, in Table 4, Panel B, column 1 implies that the additional maintenance due to political incentives increases major injuries by 0.266. These additional major injuries are shown in line 1 of Panel D in Table B.1.

The price of additional major injuries is provided by the CDC WISQARS database. As shown by Panel D lines 2 and 3, the medical costs of a major injury are $60,461 and the lost work costs of a major injury are $138,638. This means that the additional major injuries in car crashes from mayoral elections cost $52,960 which is shown in Panel E, line 2.

B.3 Cost Aggregations

Finally, it is necessary to aggregate the medical and maintenance costs for one mayoral election in Pittsburgh. This occurs in several steps, first adding together the medical and maintenance costs for one election in Pittsburgh. Combining the optimal paving deviation costs and increase in major injuries due to car crashes (summarized in Panel E of Table B.1) means that one local election in Pittsburgh costs a total of $90,623 due to politically-motivated maintenance shifts. This is shown in line 1 of Panel G in Table B.1. Furthermore, the data in this paper spans three mayoral elections, so it is internally valid to expand these costs across three mayoral elections in Pittsburgh to a cost of $271,869, which is shown in Panel G, line 2.

The issue with stopping at this figure is that local elections and pavement maintenance are both widespread and frequent in the US. To give a sense of aggregate costs, the results are assumed to be externally valid to other cities in the US and time periods.Assuming external validity to other cities is a conservative assumption, because other large cities tend to be more politically competitive than Pittsburgh. This is a fair assumption, especially given that Pittsburgh has not had a Republican mayor since 1934, and the nine person city council has been entirely Democratic since 1933. If cities are more politically competitive, then it stands to reason that the political incentives to make larger shifts than Pittsburgh are present. This is the reason that we believe that using these estimates across other cities creates a lower bound of the cost of mayoral elections due to maintenance shifts.

To understand how much potential there is for under-estimation of costs due to more political competition in other cities, compared to the Pittsburgh baseline, data on party of mayors are gathered going back to 1960.[25] Table B.2 lists the number of Republican mayors for each of the forty-seven cities from the FHWA list.[26] Of the forty-seven cities on this list, thirty-one have had at least one Republican mayor, implying they are more politically competitive than Pittsburgh.[27] This supports the claim the cost estimates are conservative when multiplied across other similar cities in the US.

One potential drawback to using Pittsburgh data as an estimate for other cities' costs caused by local election cycles is that other cities may not be governed under the same mayor-city council system. If other cities do not share this governance structure, then the mayor would probably have considerably less power (or no power) in these cities to determine the timing and location of road maintenance projects at the city level. In the US, though, the majority of the most populous cities do have a mayor-city council system.[28]

Panel F of Table B.1 describes the candidate pool of cities which are used to aggregate costs to the national level. First, only US cities are included to limit governance and transportation system differences. Next, only fairly large cities in terms of population are used, since city population and total road length are positively associated. Line 1 of Panel F shows that there are 310 cities in the US with a population of at least 100,000 people in 2020 (World Population Review, 2020). The largest cities, with population of at least 750,000, in line 2 of Panel F come from the Federal Highway Administration Database (FHWA). Finally, in line 3 of Panel F, the large cities are subset down to those with similar population density and road length using FHWA statistics.[29]

Obviously, there are several ways to aggregate the maintenance and major injury costs up from the single election cost in one city in Panel G line 1, but we consider three separate scenarios in

[25]Data were collected from city websites and publicly available information. Some cities have incomplete information regarding the political party of their mayor over time. Nevertheless, this constructed variable shows the variability of cities relative political "competitiveness" across the county.

[26]Number of Republican mayors is an appropriate approximation of political competition because the closer this number is to the midpoint of the number of local elections since 1960 (0,15), the more times the office has changed party control.

[27]The average number of Republican mayors is 2.33, suggesting that some other cities may be orders of magnitude more competitive than Pittsburgh.

[28]See `https://www.nlc.org/resource/forms-of-municipal-government/` for more details.

[29]Similar is within 2 standard deviations of average for all the following variables: population, population density, total highway miles, highway miles per capita, highway miles travelled per capita, average annual daily traffic (AADT), etc. The excluded cities and the variable that makes them different are summarized in column 2 of Table B.2.

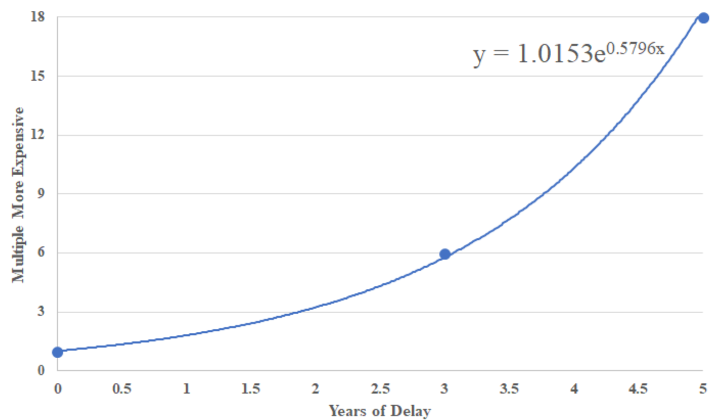
lines 3-5 of Panel G in Table B.1. First, for one mayoral election in the most similar 37 large cities, the cost is $3,353,057. Next, across all medium-large cities, one mayoral election costs $28,093,182. Last, mayoral elections are relatively frequent so the costs are extrapolated back in time. One reasonable way to do this is to go back to the second of three periods of US road construction, as defined by Glaeser and Ponzetto (2018), which begins in 1960. For all medium-large cities since 1960, which encompasses 15 mayoral elections over 60 years, the maintenance and car crash costs of local elections are equal to $421,397,740.

TABLE B.1: SUMMARY OF COSTS OF LOCAL ELECTIONS

Panel A: Maintenance Shifts	
June-August	-605.76
October	565.48
Total Not Completed	-40.28
Panel B: Maintenance Costs	
Per Meter	$ 45.55
Multiple More Expensive (Per Delayed Year)	1.0153*(exp(.5796*Y))
Panel C: Assumptions on Y	
For 565.48 meters	0.33
Leftover 40.28 meters	2
Panel D: Crash Costs of Major Injury	
Additional Major Injuries	0.266
Medical	$ 60,461.00
Work Loss	$ 138,638.00
Panel E: Costs Summary	
Deviation from Optimal Paving	$ 37,662.84
Major Injuries Increase	$ 52,960.33
Panel F: Numbers of Comparable Cities	
Medium (Population $\geq$ 100,000)	310
Large (Population $\geq$ 750,000)	47
Large (Similar Roads and Urban Pop.)	37
Panel G: Scenarios	
Pittsburgh Only, 1 Election	$ 90,623.17
Pittsburgh Only, 3 Elections	$ 271,869.51
Most Similar Large Cities, 1 Election	$ 3,353,057.29
Medium-Large Cities, 1 Election	$ 28,093,182.67
Medium-Large Cities Since 1960, 15 Elections	$ 421,397,740.10

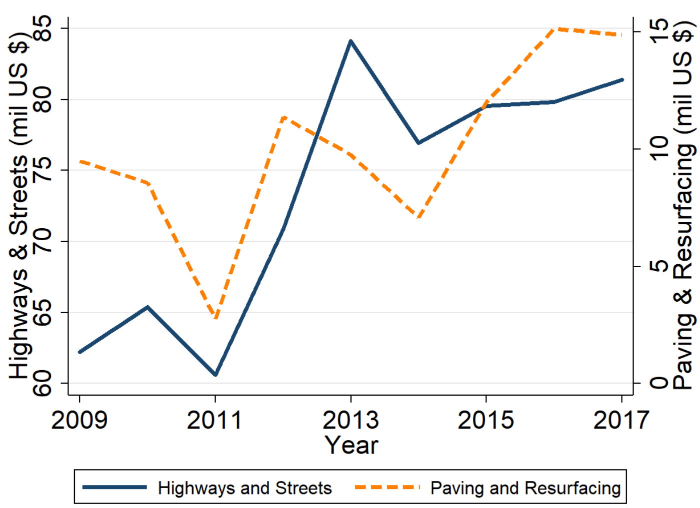
Notes: Table summarizes the cost calculations of political incentives on road maintenance related outcomes. Panel A comes from Table 3. In Panel B, the average per-meter maintenance costs for the city of Pittsburgh are derived by taking the total paving expenditures in Figure B.2 and dividing by the total length of maintenance. Then, the average across years is calculated, weighted by the expenditure amount in each year. The cost delay function comes from Burningham and Stankevich (2005). Panel D estimate of additional injuries comes from Table 4. Cost figures in Panel D are taken from the Centers for Disease Control and Prevention's (CDC's) WISQARS database and are reported in 2017 dollars. Panel F comparable cities come from the Federal Highway Administration's list of urban areas and World Population Review (2020).

FIGURE B.1: COST OF ROAD MAINTENANCE DELAYS



Note: Exponential function extrapolated from two points which are noted in Burningham and Stankevich (2005). We added a point at (0,1), because it is likely there is no extra cost from no delay. This reduces the R-squared from 1 to 0.9995.

FIGURE B.2: SPENDING ON ROADS BY YEAR IN PITTSBURGH



Note: Graph charts the expenditures on roads and road maintenance in the city of Pittsburgh for the length of the sample (2009-2017). The navy line represents total expenditures on highways and streets (construction, repair, etc.), while the orange-dashed line represents the expenditures just on paving and resurfacing. Sources: Pittsburgh Comprehensive Annual Financial Reports (`https://pittsburghpa.gov/controller/cafr`) and Pittsburgh Capital Budgets (`https://pittsburghpa.gov/council/capital-budgets`)

TABLE B.2: FEDERAL HIGHWAY ADMINISTRATION STATE & URBANIZED AREA STATISTICS: POPULATION OVER 750,000

Urban Area	Variable Outside of Two SD from Mean	Number of Republican Mayors Since 1960
New York Northeastern NJ, NY	Urbanized Population ↑	3
Los Angeles, CA	Urbanized Population ↑	2
Chicago - Northwestern IN, IL	Federal-Aid Urbanized Land Area ↑	0
Philadelphia, PA		0
San Francisco - Oakland, CA		1
Detroit, MI		1
Dallas - Ft. Worth, TX		4
Washington, DC		0
Atlanta, GA	Daily Vehicle Miles per Capita ↑	0
Boston, MA		0
San Diego, CA		8
Houston, TX	Daily Vehicle Miles per Capita ↑	2
Minneapolis - St. Paul, MN		2
Miami - Hialeah, FL	Persons per Square Mile ↑	6
Phoenix, AZ		7
Baltimore, MD		1
St. Louis, MO		0
Seattle, WA		1
Denver, CO		1
Tampa - St. Petersburg, FL	% of Travel Served by Freeways ↓	3
Cleveland, OH		2
San Jose, CA		0
Ft. Lauderdale - Hollywood, FL		1
Pittsburgh, PA		**0**
Milwaukee, WI		0
Norfolk - VA Beach - Newport News, VA		0
Kansas City, MO	Total Freeway Miles per Urbanized Population ↑	1
Sacramento, CA		0
Riverside - San Bernardino, CA		1
Portland - Vancouver, OR		1
San Juan, PR	Daily Vehicle Miles per Capita ↓	NA
Las Vegas, NV		1
Cincinnati, OH		4
Orlando, FL		2
San Antonio, TX		1
Buffalo - Niagara Falls, NY		1
Oklahoma City, OK		5
New Orleans, LA	Daily Vehicle Miles per Capita ↓	0
W. Palm Beach - Boca Raton - Delray Bch, FL		0
Columbus, OH		4
Memphis, TN		0
Indianapolis, IN		4
Providence - Pawtucket, RI		0
Jacksonville, FL		3
Salt Lake City, UT		2
Louisville, KY		2
Tulsa, OK		7

Note: Urbanized areas in the United States with populations above 750,000 people, listed in order of population. ↑: two standard deviations above the mean of all cities listed ↓: two standard deviations below the mean of all cities listed. Source: Federal Highway Administration (https://www.fhwa.dot.gov/ohim/onh00/onh2p11.htm) Number of Republican mayors since 1960 measures the relative political "competitiveness" of the cities on the list.